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MSS. intended for publication and books, etc., intended for review should be sent to the responsible editor, Professor J. McKeen Cattell, Garrison-on-Hudson, N. Y.

*CERTAIN RECENT ATTEMPTS TO TEST THE NEBULAR HYPOTHESIS.**

IT is a far cry from the glacial period to the nebular hypothesis, but yet it is not beyond the view hulloa of logic. Glacial periods have certainly been dependent on atmospheric states, whatever else may have been concerned in causing them. Surely no one will imagine glaciation in the air of the putative molten earth, nor in the warm dense atmosphere currently assigned to the early ages, nor yet in the later periods when figs and magnolias grew in Greenland. If carbon dioxide has the thermal qualities which eminent physicists assign it, continental glaciation could scarcely have occurred while it was a large constituent of the atmosphere. Now the atmosphere has,

* This paper, prepared at the request of the editor of SCIENCE, is little more than an abstract of the following three papers:

I. 'A Group of Hypotheses bearing on Climatic Changes,' by T. C. Chamberlin; *Journal of Geology*, Vol. V., No. 7, 1897, pp. 653–683.

II. 'An Attempt to test the Nebular Hypothesis by the Relations of Masses and Momenta,' by T. C. Chamberlin; *Journal of Geology*, Vol. VIII., No. 1, January–February, 1900, pp. 58–73.

III. 'An Attempt to Test the Nebular Hypothesis by an Appeal to the Laws of Dynamics,' by F. R. Moulton; *Astrophysical Journal*, Vol. XI., No. 2, March, 1900, pp. 103–130.

By 'nebular hypothesis' the gaseous hypothesis of Laplace is always to be understood in this article. The arguments, for the greater part, apply also to all spheroidal hypotheses in convective equilibrium, whether gaseous or meteoroidal.

during its history, contained many thousands of times its present amount of carbon dioxide, as is implied by the vast stores of carbon and carbonates that have been removed from it. We are thus driven to assume either that the early atmosphere was very rich in carbon dioxide, and has been impoverished as the ages have gone on, or that the loss has been made good by supplies received concurrently. In the former case glaciation and similar phenomena dependent on an impoverished atmosphere should be confined to the later ages. But the most extraordinary glaciation of which we have any knowledge took place near the close of the Carboniferous period, or, in other words, far back in the geological series. Vast beds of limestone, coal, and other carbonaceous deposits have since been formed at the expense of the atmosphere's carbon dioxide. Much oxygen and some nitrogen have also been consumed, but we need not dwell on these. If all this carbon dioxide had been in the atmosphere at that time there is reason to doubt whether the glaciation of India, Australia and South Africa could have taken place. Besides, in the Permian and Triassic periods great salt and gypsum beds were laid down over many degrees of latitude and longitude on both continents. These imply an aridity of surprising extent, duration, and intensity, reaching to latitudes not at present affected by appreciable excess of evaporation over precipitation. If the atmosphere had been rich in carbon dioxide, which is believed to equalize temperature and humidity, there is reason to doubt whether these deposits could have been formed. But even still earlier, as far back as Silurian times, before the coal of the Carboniferous period or the carbon dioxide of the great limestones of the Subcarboniferous and the Devonian times had been taken out of the atmosphere, widespread and thick salt beds were formed in the St. Lawrence basin where now the excess of precipitation forms great lakes and a mighty river. Nor is even this the earliest evidence of notable aridity.

Now these and allied phenomena, which imply extraordinary inequalities of atmospheric states, call for a reconsideration of inherited views regarding the constitutional history of the atmosphere. To suppose simply that the atmosphere was once exceedingly rich in carbon dioxide and has been steadily impoverished does not seem to fit the phenomena. But when once a reconsideration is undertaken there is no stopping place short of the original state of the atmosphere, and we are at once involved in the *pros* and *cons* of the nebular hypothesis.

If the nebular hypothesis is approached from the atmospheric side, we must carry into the inquiry the modern kinetic theory of gases or give reasons for dissenting from its validity. In framing the nebular hypothesis a century ago Laplace could not call to his aid the present dynamic conception of gases, and, while this absolves him from responsibility, it makes it the more fitting that the hypothesis should be tested by the kinetic views that have grown out of researches since his day. These views may perhaps require modification in the future, but such modification is more likely to involve intensified molecular activity than the opposite.

Led thus to the subject, we have attempted to make a test of the nebular hypothesis by a comparison of the molecular velocities of the essential gases with the gravitative power of the earth, and its antecedent nebulous ring, to control these at the temperatures assigned them by the hypothesis. We have followed the general method employed by Dr. Johnstone Stoney in discussing the atmospheres of planets and satellites.* The essence of the method is

* 'On the Cause of the Absence of Hydrogen from the Earth's Atmosphere and of Air and Water from

this: The molecules of gases beat upon each other and rebound with great frequency and high velocities. Both the frequency and the velocities rise with temperature. The molecular velocity of a gas is *inversely proportional* to the square root of its molecular weight and hence for the lighter gases it is very high. The velocity of a particular molecule at any instant depends on the nature of the last previous collision, being sometimes much higher than the average velocity and sometimes much lower. Now it is obvious that if a molecule on the outer border of the atmosphere collides with another and rebounds outwards with a velocity so great that the attraction of the earth cannot overcome it, the molecule will fly away into space and be lost. Dr. Stoney has attempted to show that on account of the high velocities thus frequently attained, hydrogen and even helium are not permanently retained by the earth under present conditions. He has also endeavored to prove that the moon and other small bodies cannot retain any of the atmospheric gases, and that this is the reason they are without atmospheres, and this latter view is now quite generally accepted.

Mr. S. R. Cook, however, has attempted to show by mathematical analysis that the rate of loss of hydrogen from the earth is at present too small to be effective,* but he has based his computations on the theoretical parabolic velocity of the earth and not on its available power of control in competition with the sun and other bodies of the solar system, and he also neglected the ionization of the gases and the peculiar conditions of the upper atmosphere.* We have endeavored to show that whatever doubt there may be about the precise competency of molecular velocities to cause loss of the lighter gases at present, their retention would be put in jeopardy if the temperature of the earth were raised to 3500° or 4000° C. as would be necessary to restore the earth to the original gaseous condition postulated by the Laplacian hypothesis. At such temperatures water would be dissociated into hydrogen and oxygen, if not ionized to a higher degree; the molecular velocities of these gases would be exalted by the intense heat† and, in such a state of gaseous extension, the outer part would be far from the center of gravity where the control would be enfeebled.‡ Under such conditions it seems highly improbable that hydrogen could be retained, and hence, the inference that if the earth had passed through such a history it would be deficient in hydrogen compounds. Not only the atmosphere but the ocean would seem to be put in jeopardy.

But this is by no means the crucial application of the test. The Laplacian hypothesis assumes that the material of the earth and moon was detached from the solar mass as an equatorial ring whose diameter was essentially that of the earth's orbit. Now the gravitation at any point on the surface of such a ring would be very feeble —very much feebler indeed than that on the present surface of the moon where no atmosphere is retained. At the same time, by hypothesis, the temperature of the ring was very high, and this high temperature would only prevail if there were frequent and intense collisions. But the rebound from such intense collisions would carry

the Moon,' by Dr. G. Johnstone Stoney, Royal Dublin Society, 1892, and 'Of the Atmospheres upon Planets and Satellites,' by G. Johnstone Stoney. Trans. Royal Dublin Society, Vol. IV., Part B, Oct., 1897. See also Paper I. above cited, pp. 653–683.

* 'On the Escape of Gases from Planetary Atmospheres according to the Kinetic Theory,' by S. R. Cook. *Astrophysical Journal*, Vol. XI., No. 1, January, 1900, pp. 36–43.

* See reply of Dr. Stoney 'On the Escape of Gases from Planetary Atmospheres according to the Kinetic Theory.' J. G. Johnstone Stoney, *Astrophysical Journal*, p. 251, May, 1900. *Ibid.*, II., June, 1900, p. 357.

† See table, Paper I , above cited, p. 661.

‡ Paper I , pp. 659–661.

the molecules beyond the control of the feeble gravity of the ring, and its dispersion and cooling would seem to be inevitable.* There seems therefore no good ground for supposing that such a ring could maintain either its coherence or its temperature.

But if the ring were dispersed and cooled might it not be reheated to the gaseous condition in subsequently collecting into the globular form? Although a rigorous demonstration is beyond the reach of present mathematical processes, it is possible to make a sufficient approach to a valid conclusion respecting the rate at which such a ring would collect into a globe as to render it improbable that it would heat itself to the requisite temperature or any close approach to it. It is indeed a question whether aggregation would take place at all as the direct result of its own gravitation. Bearing upon this, one of us has attempted to solve a series of specific cases purposely made most favorable for aggregation.† It was assumed, regardless of the probabilities, that an aggregation had already progressed so far as to form a large body having essentially the full gravitative power of the earth and yet it seemed improbable that this body could bring to itself infinitesimal particles from portions of the ring more than 60° distant from itself in heliocentric longitude, unless this were accomplished by other than the simple gravitative force of the earth, the sun and these particles. From this it is concluded that the traditional idea of a hot gaseous ring breaking at some point and gathering into a gaseous globe while still hot enough to maintain the refractory substances of the earth in a gaseous condition, is not tenable, both because of the molecular difficulties and the gravitative incompetencies.

Pursuing this line further, we have inquired whether any single or dominant condensation would take place in a ring of tolerable homogeneity.* Students of the subject are aware that the rings of Saturn are composed of particles of discrete nongaseous matter and cannot aggregate into satellites because of the differential attraction, or tidal strain, of the planet. They do not illustrate gaseous rings of the Laplacian type on the way to the formation of satellites as once supposed, but quite the reverse. If satellites of equal masses were substituted for them, they would be torn into fragments by the tidal pull of Saturn, and probably redistributed into meteoric rings. The rings appear to represent a state of equilibrium and not a state from which rapid aggregation should naturally proceed, as assumed in the case of the Laplacian rings. The limiting distance within which this power of disruption is exercised by the planet is dependent on its gravitative power and is known as *Roche's limit.* For Saturn it lies a little outside of the outer ring; for the earth, according to G. H. Darwin, it lies about 11,000 miles from the earth's center. We attempted to apply and extend the principles of Roche to cases arising under the nebular hypothesis and in the course of this devised a new criterion of similar nature, applicable to attenuated gases in the form of ellipsoids and rings such as are postulated in the Laplacian hypothesis. For the precise nature of this the reader must be referred to the original paper.† It will suffice here to say that while Roche determined the limits, under assumed conditions, within which disruption would take place, the new criterion assigns the limits, under assumed conditions, within which the aggregation of attenuated or dispersed matter would *not* take place as the result of its own gravitation, in the presence of the superior differential gravitation of the sun. The conclu-

* Paper II., before cited, pp. 658–665.

† Paper III., before cited, pp. 115–117.

* Paper III., above cited, pp. 118–129.

† Paper III., above cited, pp. 122–126.

sion reached by this inquiry was that a Laplacian ring could not have contracted directly into a gaseous globe, and that the nebula out of which the solar system was evolved must have been one of great heterogeneity rather than one of the pronounced homogeneity assumed in the Laplacian hypothesis.

The further question whether the equatorial matter of a spheroid of gas whose rotation was increasing would separate intermittently as rings or go off continuously is not new, but it was thought worth while to reconsider it in the light of modern conceptions of the outer border of an atmosphere or of a globe of gas. This outer border is not now regarded as a defined surface where gravity and 'repulsive force' balance; on the contrary, the outer portion is somewhat like a fountain in which individual molecules are thrown by the rebound from collisions to varying heights, from which they return in elliptical paths, possibly to be thrown back again or to assist in projecting other particles through like paths. There is no theoretical limit to the extent of these excursions short of escape from the control of the main body. The actions of the molecules in this outer portion are therefore more individual and free than those of the denser mass, and in the course of their long free curving paths they may collide in such a way as to become satellites to the main body.

Now the extreme tenuity of the Laplacian nebula seems not to have been considered in connection with ring-formation. One of us has computed that the *average* density of the solar nebula, when extended to the orbit of Neptune, would be 1/191,000,000,000 of that of water.* The tenuity of the extreme outer portion must therefore have been quite beyond the limits of the imagination. In view of this extreme tenuity and the peculiar constitution already cited, it is scarcely possible that there could have been any effective cohesion to prevent the separation of the peripheral portion particle by particle as the individual centrifugal force of each came to equal the centripetal force. It is clear that in a mass of gas densest at the center the centrifugal force would overtake the centripetal force first at the equatorial surface.* The conclusion is therefore that the peripheral matter would have been left behind continuously and that separate rings would not have formed.

Some minor arguments that merely touch the probability of the Laplacian hypothesis may be passed by.†

Arguments of the foregoing class, though they seem entitled to great weight, lack something in rigor, for, at present, exhaustive data cannot be commanded and treated by precise mathematical methods. We, therefore, had recourse to lines of attack of a more mechanical sort. These were found in the relations of mass and momenta. We attempted (1) a comparison of the moment of momentum of the supposed nebular system with the moment of momentum of the actual system, and (2) a study of the ratios of masses to momenta.

1. It is a firmly established law of mechanics that any system of particles rotating about a common axis retains a constant moment of momentum whatever change of form may take place as the result of its own evolution. The evolution of the solar system under the Laplacian hypothesis is such a case. If, therefore, we can restore, theoretically, the supposed nebulous system and compute its moment of momentum, it must be found at all stages the same as at present. The only serious difficulty of the method lies in determining the distribution of density through the postulated nebulous mass. Fortunately this has been at-

* Paper III., above cited, pp. 114.

* Paper III., above cited, pp. 114, 115.

† Paper III., pp. 107–111.

tempted, under some limitations regarding the motions, by some of the ablest of mathematicians and physicists, among whom are Lane, Ritter, G. W. Hill, George H. Darwin, and Lord Kelvin.* The results reached by all are in substantial agreement, though somewhat different analytical methods were followed. The distribution of density computed by Darwin was used in our computations.

The present moment of momentum of the whole system, sun, planets and satellites included, was found to be 22.7666, reckoning the sun as homogeneous, which gives too large results but favors the nebular hypothesis. The unit is a convenient arbitrary one. The moment of momentum of the solar nebula when it reached the orbit of Neptune and had the angular velocity of Neptune, which would be necessary to separate the Neptunian ring, was by computation 4848.055.† These momenta, which should be equal, stand in the ratio of 1 : 213. Furthermore the ratios are different at different stages of the evolution; for example, for the stage just preceding the separation of the earth the ratio of the nebular momentum to the actual momentum was found to be 1 to 1208, and for the stage just before the separation of Mercury, 1 to 754. Larger discrepancies would have been found if the Laplacian hypothesis had not been given the benefit of every doubt as to the structure and of all margins in computation. If for example, the sun be assigned an increase of density toward the center, according to Laplace's law, which is probably near the truth, the last two ratios would be 1 to 1801, and 1 to 1127, instead of the figures given.

For a discussion of the question whether these discrepancies can be due to a radical error in the law of density, the reader must be referred to the original paper.* It can only be stated here that the probable variations from the accepted law of density seem rather more likely to increase the discrepancies than to diminish them, and further that the discrepancies are so enormous that the law must be supposed to quite break down to bring them into harmony. Furthermore it must break down irregularly, for the figures run

Neptunian stage	213
Jovian stage	141
Terrestrial stage	1208
Mercurial stage	754
Present stage	1

To satisfy the laws of mechanics all these should be unity.

2. As the foregoing comparison involves the distribution of density in the supposed gaseous nebula, concerning which there is some doubt, it was obviously desirable to find some mode of comparison which should not involve this factor. This was sought in a comparison between the ratios of the planetary masses to their parent nebula, and the ratios of the planetary momenta to the nebular momenta. In this case the nebular momenta were obtained by adding together the component planetary momenta which they must have equaled under the laws of mechanics. The momenta of the satellites were reckoned in with their respective

* Lane, 'On the Theoretical Temperature of the Sun under the Hypothesis of a Gaseous Mass maintaining its Volume by Internal Heat, and depending on the Laws of Gases as known to Terrestrial Experiments.' *Am. Jour. Sci.*, Vol. XLIX., pp. 56–74, 1870.

Ritter, 'Untersuchen über die Höhe der Atmosphäre und die Constitution gasförmiges Weltkorper,' *Wiedmann's Annalen*, New Series, Vol. LXVI., 1882, p. 166.

G. W. Hill, 'Annals of Mathematics,' Vol. IV., 1888.

Darwin, 'On the Mechanical Condition of a Swarm of Meteorites, and on the Theories of Cosmogony.' *Trans. Phil. Soc.*, 1888.

Kelvin, 'On the Origin and Total Amount of the Sun's Heat,' Popular Lectures and Addresses, 1891. *Constitution of Matter*, pp. 370–429.

† Paper III., pp. 127–128 and Paper II., p. 64.

* Paper II., pp. 65–67.

planets, the estimates of Darwin being used throughout.

Just previous to the supposed separation of the Jovian ring, the moment of momentum of the parent nebula, reckoned from the present moments of momenta of the bodies derived from it, was 14.1816. Now Jupiter has 13.469, or about 95%, of this. But the mass of Jupiter is only 1/1047 of the parent nebula, or less than one-tenth of one per cent. Neglecting for the moment any transfers of momentum that may have taken place afterwards, it appears that, by hypothesis, *the Jovian ring carried away less than one-thousandth of the mass of the nebula, while at the same time it took off 95% of the moment of momentum.* Is such a thing possible in a gaseous spheroid evolving under gaseous laws, or evolving in any form of convective equilibrium? One nineteen-thousandth more of the mass thrown off with an equal proportion of momentum would have left none in the central body!

A similar comparison in the case of the other planets reveals not only very extraordinary ratios but such large and irregular variations in the ratios as could hardly be expected in the systematic evolution of a gaseous body.

To the inquiry whether these discrepancies can be due to subsequent transfers of momentum by tidal friction, the computations of G. H. Darwin have given an emphatic negative; and these are supported by other considerations.*

The general conclusion from these several attempts to test the nebular hypothesis of Laplace is altogether adverse to its tenability. It is equally adverse to any meteoroidal hypothesis which assumes a quasi-gaseous behavior, or an aggregation controlled by the laws of convective equilibrium, as set forth by G. H. Darwin in his memoir 'On the Mechanical Conditions of a Swarm of Meteorites and on Theories of Cosmogony.' The inquiry into the relations of masses and momenta points to an unsymmetrical distribution of matter and energy quite inharmonious with an original spheroidal form of any kind. On the contrary, it seems to indicate that *the origin of the system was such that the outer part acquired all but a trivial part of the momentum while it possessed only a trivial part of the mass.* In specific terms, the outer or planetary part now embraces only about 1/700 of the mass, while it carries more than 97% of the moment of momentum. The sun has no such residual rotatory momentum as to imply that he ever 'threw off' any planets from his equator. If the solar system were converted into a gaseous nebula controlled by Boyle's law and given the existing moment of momentum and allowed to contract, the centrifugal force would not overtake the centripetal until long after the orbit of Mercury had been passed.

The ratios of masses to momenta and the discrepancies of the system clearly have a high value in the construction of a tenable hypothesis, whatever that may prove to be; for they are specific criteria which must be met. In an attempt to construct such a hypothesis, *the matter of the system must be so brought together as to give low mass, high momentum and irregular distribution to the outer part, and high mass, low momentum and sphericity to the central part.* In speculation in this direction the possibility of the initiation of the system by the peripheral collision of a very small nebula upon a large one has seemed worthy of consideration. Assuming that the collision was essentially due to mutual gravitation, the smaller nebula must, from the nature of the case, have had a relatively high velocity, and hence a high ratio of momentum to mass, while the larger nebula may have had little initial rotation, or may even have had a rotation contrary to the present one, which was reversed by the impact, or the recur-

* Paper II., pp. 70–71.

ring series of impacts, of the smaller nebula. So far as we can now see, the most serious difficulty in framing a consistent hypothesis along this line lies in the approximate circularity of the present planetary orbits, but as circularity may result from the combination of a large number of constituents having elliptical orbits, this difficulty may not prove insuperable.

We naturally turn to the heavens for nebulæ whose evolution might give a system of low mass and high momentum in the outer part and high mass and low momentum in the central part. The spiral nebulæ offer the greatest promise of conforming to these demands for they seem to present attenuated outer matter irregularly dispersed and perhaps in relatively high motion, while the central portions are usually denser and seem to possess less momentum relatively, but this is little more than pure conjecture based on their forms, for nothing is positively known of the dynamics of these masses. Professor Keeler has shown by recent photographic researches that spiral nebulæ are the dominant forms among the smaller class. This justifies us in giving them precedence in attempts to find analogies for the origin of our system. This suggestion may really be identical with the preceding, for, in the absence of any knowledge of the origin of spiral nebulæ, it is possible to conjecture that they arose from peripheral collisions of antecedent nebulæ.

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July 9, 1900.

THE ILLUSORY DUST DRIFT. A CURIOUS OPTICAL PHENOMENON.

IT is of course improbable in the highest degree that the phenomenon here to be described has entirely escaped notice hitherto, but the writer at least is unaware of any existing description of the same. The conditions under which the illusion arises are so easy to fulfill, and the resulting appearance is so odd in many ways, that the readers of SCIENCE may be interested in a brief description of the matter. The only 'apparatus' required is a set of black and white lines and a dark background near by. The best results, perhaps, are obtained by using a square yard of common black cloth bearing narrow white lines not more than two millimeters apart. Such cloth may be obtained at any large dry goods store. If now this be hung upon the wall in a strong light, and a square of dull black cardboard be placed above it, or at the side, everything is ready for the observation. Picking out some point near the center of the cloth, let this be fixated steadily for not less than twenty seconds. Then transfer the gaze quickly to the black cardboard, and the illusory dust drift will appear. The appearance is that of *a thin cloud of fine white dust moving across the field of vision.* Or the tiny particles seen may be likened to the motes in a sunbeam, since they much resemble these in density. A steady fixation of the eyes is at no time absolutely essential. They may roam freely over the cloth and then later over the dark background, though the illusion under these circumstances is diminished in strength. The best results are unquestionably secured by as resolute a fixation of the cloth as possible. The necessary *duration of this fixation* seems to depend upon the retinal sensitiveness of the observer. Probably 5 sec. is the minimum for any noticeable after effects, while no advantage seems to be gained in any direction by prolonging the fixation beyond a period of 30 sec. In practice, successive renewals of the illusion may be accomplished by very brief fixations, provided only that the time of the first fixation be moderately long.

The *duration of the illusion* seems also to be an individual matter. One observer can